COMITATO ORDINATORE
PER IL IX CONGRESSO INTERNAZIONALE DI GENETICA

~~Segreteria Generale: Istituto di Genetica dell'Università~~
~~Via Celoria 10 - Milano (Italia)~~

16/2/52

Dear Lederberg,

I am enclosing a letter from Hayes - could you return it to me later ? . I am also enclosing copy of my letter to him.

Unfortunately my activity is somewhat impaired by organizing duties of various kinds ; but I have kept experimenting inspite of it. I have concentrated on the nature of the transforming principle for fertility. There probably is some possibility of isolating it from cells by means of heating at 60^{9} for less than 1^{h}; but the results have been doubtful. Filtering cells grown with citrate, arsenate, or thyosulphate, to prevent DNA-ase or oxidation have been fruitless. I have now a fresh hope, by grinding cells at low temperatures, since it seems that DNA-ase greatly reduces infection by F+, and it may be therefore that the transforming principle is an insoluble, or exceedingly unstable DNA. The experiment with DNA-ase was exciting : infection on 4 hours exposure of logarithmically growing cells reached 90% , while with DNA-ase it was less than 20%. I have used a relatively raw DNA-ase, which I had to prepare myself. I am waiting for purer samples.

Hfr may give you some surprise on testing for F+ potency. I have not tested carefully neither this nor its Hfr property; but experiments on these points were unlucky, and I am sending you the best subculture I have. I shall isolate more later and shall let you have them. In the meanwhile I should not like to keep you waiting any longer.

I am rather anxious to hear your news. If the DNA-ase story is confirmed - I imagine you will have tried the same yourself, the whole story is somewhat clearer. There remain a number of obscure points however. I wonder if, things standing as they are, some preliminary note might be published ? And if so, would you suggest (i) a joint note, (ii) separate notes, perhaps on the same issue of some journal like Nature , (iii) anything else that you might prefer.

As to your strains , for which I have not properly thanked you in my last letter : 58 was sterile , but the problem of B- is not very interesting; also, BM- does not seem to contain important chromosome changes so that it is not important to analyse the steps. The others are under test now. W 1817 (I have assumed that it be a TLB_1-, but have not checked directly) gives some prototrophs with W 1802, though less than I usually get with F+ infected TLB_1-. I have not checked those prototrophs. I am very interested to hear that the step at which TLB_1- mutated from F+ to F- has been located between T and L.

Hoping to hear from you soon

Yours

Cavalli